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DONALDSON

REPLY TO THE CALUMNIES  
AND MISREPRESENTATIONS  
OF PROFESSOR T. H. KEY



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# A REPLY

J. H.  
III. 15.  
1844.

TO THE

CALUMNIES AND MISREPRESENTATIONS

OF

PROFESSOR T. H. KEY.

BY THE

REV. JOHN WILLIAM DONALDSON, B. D.

AUTHOR OF

VARRONIANUS, THE NEW CRATYLUS, &c &c.

κερδοὶ δὲ τί μάλα τοῦτο κερδαλέον τελέθει;  
ἄτε γὰρ εἰνάλιον πόνον ἐχοίσας βαθὺν  
σκευᾶς ἐτέρας, ἀβάπτιστός εἰμι, φελλὸς ὥς ὑπὲρ ἔρκος, ἄλμος.

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## A R E P L Y,

ETC.

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MR. T. H. KEY, a philologer whose merits, so far as they have been known to me, I have always most willingly acknowledged, has reprinted, with certain additions, a number of articles which, it seems, he contributed to the *Penny Cyclopædia*. In a prefatory letter, which he has addressed to my friend Mr. Long, he indulges in a violent tirade against a book called *Varronianus*, which I had published about a fortnight before the date of his letter. *Καὶ κεραμεὺς κεραμεῖ κοτέει καὶ τέκτονι τέκτων*, is an adage which I would rather see justified by an adversary than by myself; and if Mr. Key had contented himself with his attempt to disparage me, I should not have noticed his attack. But he is not satisfied with vilification. He has the presumption to maintain that I am indebted to him, though I have not acknowledged my obligations, for all the little good which he can find in *Varronianus*. This is an assertion the truth of which I feel myself bound to controvert. Censure and criticism may be left to themselves; but distinct accusations, however frivolous, eager, and inconsiderate, must be met by exposure and confutation.

Mr. Key's attack is directed chiefly against the 7th and 10th chapters of *Varronianus*, of which (he says, in p. 12) I have "borrowed a great part from the productions of one English writer," namely, himself; and the chapters referred to are severally accused in pp. 2 and 6.

Before I proceed to examine this calumny in detail, I must beg leave to make a few remarks on the manner in which scholars in general have thought themselves entitled to use the labours of their predecessors and contemporaries. In regard to original results, general principles, emendations of corrupt passages, every thing, in fact, which amounts to

discovery, it is held that the most scrupulous references ought to be made to the source from which the information is derived. But with regard to the mere raw materials of scholarship—quotations, illustrations, lexicography, tabular comparisons, &c., it is held that every scholar is entitled to consider them as the advantages with which he was born—his τὰ ὑπάρχοντα, his starting point, the ἐπιβύσεις τε καὶ ὁρμαὶ of his own investigations. This principle was distinctly avowed by a very eminent scholar, Dr. Blomfield, the present Bishop of London, when he was obliged to defend himself from a charge of plagiarism brought against him by a veteran performer of the part in which Mr. Key has just made his *début*. “Once for all, I observe,” says his Lordship (*Museum Criticum*, ii. p. 499), “that I never did nor do at present conceive, that in quoting an author in the way of illustration (not of emendation), a critic is bound to mention all the preceding scholars who have quoted him for the same purpose. . . . It has not hitherto been considered necessary. Porson uses without scruple the same passages which had been adduced by Barnes, Valckenaer, Musgrave, and others, without mentioning their names;” and then he gives various instances. Upon this principle, which applies with still greater force to the case of those verbal analogies so common in books on comparative philology, I have invariably acted and professed to act. Those who have read my prefaces are aware that I have always professed to avail myself of all existing materials; and while I have been guided by my own judgment as to the occasions on which I should make a direct and express citation from another author rather than an indirect and tacit reference to his work,\* it will be found, I conceive, that in all my works I have rather exceeded than fallen short in my appeal to authorities. Mr. Key has perceived this (p. 4), but is unable to understand why “a book so overloaded with the names of German, French, Italian, and other writers,” should not make more frequent mention of himself. I shall find him a reason in the sequel, which, I fear, will not be very satisfactory to

\* Sometimes, indeed, the direct reference is impossible: for those who read much, and have tenacious memories, carry about with them a large stock of information, which they could not, if they wished it, distribute among the authors from whom the separate parts of it were derived.

his "personal vanity" (p. 5). But to return to the principle of tacit reference; it is a very common practice with scholars to content themselves with this notice of the works of those who have gone over the same ground with themselves, and who may be considered as, in some measure, their literary predecessors. Very many, whom I could adduce, have applied this rule in larger measure than I have ever ventured to do. How often has Porson mentioned his rival Hermann? Although the Bishop of St. David's was well acquainted with Mitford; although he wrote his *History of Greece* with Mitford constantly before him, and must have been indebted more or less to his predecessor, when and how often does he mention him by name, or refer to his volumes? Does Dr. C. Wordsworth's *Athens and Attica* swarm with references to Colonel Leake? and yet there is scarcely a page in the book in which there is not some correspondence of illustration. Nay, more, the identification of Mount Lycabettus with the hill of St. George is due to Dr. Forchhammer, whose discovery I communicated to the *Journal of Education* (ix. p. 165) in 1834: yet Dr. Wordsworth, though acquainted with Forchhammer's pamphlet (see p. 54), has given Forchhammer's discovery, and the induction on which it rests, without any reference to him. But the most cogent example is furnished by Mr. Key himself. In a passage to which I shall return before I have done with him (p. 78, note), he cites, what I consider as a discovery resting on a sufficient induction, as the imagination of "a recent writer," without any further reference to my book, or to the reasoning which has led me to my conclusion. I should have thought that in a book which commenced with an attack upon one of my works, he ought not to have made any allusion to that work without a direct citation; but whether he was right or wrong in availing himself of the principle of tacit reference in so strong a case, he has thereby, at all events, deprived himself of the right to challenge the same privilege when adopted by others.

I now come to the details of Mr. Key's misrepresentations.

I. With regard to the 7th chapter of *Varronianus*. Mr. Key, with ineffable modesty, declares (pp. 2, 3) that he feels himself debarred from speaking on the merit of that chapter, because the *great bulk of it* is borrowed from himself! and



he then proceeds to claim "an aggregate of six pages or more" as taken without acknowledgment from his *Penny-Cyclopædia* contributions. As he does not mention the number of pages in my 7th chapter, it will be considered that either his candour or his arithmetic is at fault when he calls *six* pages the *great bulk* of *thirty-seven*. But have I borrowed from him even "an aggregate of six pages or more" in my chapter of 37 pages; or am I indebted to him for any thing which demanded more than the tacit but virtual reference which I have made to his crude mass of second-hand illustrations? The 7th chapter of *Varronianus* is merely an application to the Latin language of the principles developed in the chapter on the Greek alphabet in the *New Cratylus* (pp. 96-138). Mr. Key insinuates that I was even then acquainted with his bungling approximation to my theory about the original sixteen letters. Although I have written in the *Penny Cyclopædia*, it was not till lately, not till after I had made some advance in the manuscript of *Varronianus*, that I had ready access to a copy of that miscellany. At the time of the publication of the *New Cratylus* I was very slightly acquainted with the work; and with regard to Mr. Key's article on "the Alphabet" in general, I must content myself with declaring, on the word of a gentleman, that up to this moment I have not read that article. I have, indeed, verified his reference to p. 380 of the *Penny Cyclopædia*, vol. i., and there find, what I expected, that Mr. Key has, like every body else, discovered, or heard of, some *prima-facie* traces of organic arrangement in the alphabet. For all beyond that first, tottering, and uncertain step, he must accept my guidance. I utterly deny that I am indebted to Mr. Key for a single idea in the *Varronianus*, or any other of my writings; and indeed it would be strange if I had borrowed from an author who has shewn himself so incapable of understanding the first principles of my philology. I am, however, most willing to admit, that shortly before I sent the *Varronianus* to the printer, I became acquainted with Mr. Key's articles on the separate letters, and that I used them occasionally, as I used maps, dictionaries, grammars, and other works of reference, and on the principles explained above. I admit also that I did not think these papers worthy of more



than the tacit and general references which I have made to them. Thus, in p. 192, I say: "It is generally laid down that *f* and *v* are both labio-dental aspirates, and that they differ only as the *tenuis* differs from the *medial*" (compare Mr. Key's book, pp. 60 and 104); "and one philologist has distinctly asserted their identity, meaning perhaps that in Latin *f* = the English *v*, and *v* = the English *w*." Again: p. 198, "It has been a common opinion with philologists that there were were different classes of the *tenuis* guttural, &c." (compare Mr. Key's book, p. 89). Now, it must be clear to any reader, from this mode of reference, that I did not know the author or authors of these articles in the *Penny Cyclopædia*; that I regarded these articles as mere compilations (as I still believe them to be); and that I considered them (as I still do) utterly devoid of any original merit or individuality of character. Indeed, I should experience no great difficulty in indicating the sources from which they were derived by the "useful labour" (p. 3) of Mr. Key; but I find no fault with this gentleman for availing himself of existing materials. Perhaps, indeed, he ought not to have seemed to claim as his own discovery the true analysis of the French future (pp. 123, 130), which was established by Raynouard (see Lewis on the Romance Languages, pp. 194, *sqq.*), and is well known to every writer on comparative philology (see Bopp, quoted in the *New Cratylus*, p. 454): but I should not have mentioned this, except in the case of one who has such peculiar notions about plagiarism. Now I contend, that even if I had made great use of Mr. Key's articles on the consonants, I was quite entitled to do so, in accordance with the practice of other scholars; and considering that they were anonymous, that they were not original, that they involved no new principles, but consisted only of hackneyed and commonplace illustrations, in constant and colloquial use among scholars, a direct reference to them would have been making an acknowledgment where no acknowledgment was due. But I will go a step further. Mr. Key has grossly exaggerated the use which I have made of his materials—if, indeed, they can be called *his*, more than *mine*. I will point out all the instances in which I used his papers for the purpose of suggestion. P. 191, he suggested to me Scaliger's epigram. For the

rest, see *New Cratylus*, pp. 136, 188, &c., where are some principles of philology unknown to Mr. Key, and by which I have explained *cage*, *rage*, &c., forms which Mr. Key cites, but does not elucidate. I have taken the French words in pp. 194, 203, &c., from Mr. Key, because it would have been mere affectation to go over the same ground again in such trumpery minutiae: a French dictionary, a comparative atlas, and a treatise on the Romance languages, would have furnished me with as many more as I pleased; but I preferred to indicate the last user of the materials by confining myself to his examples. Here and there another illustration or two, already known to me, may have been suggested by him; but the amount of the whole might be written down in a very short paragraph. If, by using such materials, Mr. Key has made them exclusively his own—if every philological illustration is τοῦ φθάσαντος ἀρπαγή, and is never to be used again, then happy, in comparison with Mr. Key and myself, were those scholars of the sixteenth century, who lived amidst a profusion of unappropriated materials. But I do not admit that such an employment of materials conveys any right of property in them,—otherwise Mr. Key is both a poacher and a game-preserver, and claims a right of property over the “spoils of his own forays” (p. 4).

II. Of the 10th chapter of *Varronianus*, Mr. Key ventures to assert (p. 6), that “the whole framework of” my “argument, together with all the details in support of it, are drawn (he believes he may say exclusively) from his own” two papers “on the metres of Terence.” He does not here say that I have made no acknowledgment; but he calls my note (in p. 277) “a something by way of reference.” I do not know what more I could have said than: “The reader who desires a more copious induction cannot do better than consult an excellent article on the subject in the *Journal of Education*, written, I believe, by Professor T. H. Key.” I did not refer to the article on Terentian metres in the *Penny Cyclopædia*, because it appeared to be merely a second and inferior edition of the paper in the *Journal of Education*, to which it refers, as I do, for more detailed information. From p. 277 to p. 280, any reader will perceive that I have acknowledged my obligations to Mr. Key; and I repeat, that I con-

sider his article in the *Journal of Education* an excellent induction of examples. That the reference is continuous will be seen from the top of p. 280: "The following reasons have been adduced," &c. If a handsome and complimentary reference to Mr. Key is plagiarism, I am certainly guilty here. That the rest of the chapter belongs exclusively to me; that the idea upon which it rests has been more clearly and distinctly developed by myself than by any other writer; that I have constantly dwelt upon that idea, in conversation and in teaching, for the last ten years and more; all this I would most strongly maintain, without, however, denying the merits of Mr. Key's labours in a limited department of the same field. In support of this assertion, I will seek no other testimony than that of Mr. Key himself, p. 7: "I cannot help regretting," says he, "that in adopting my view, that the French language, as regards number of syllables, more closely approaches to the Latin pronunciation than even the modern Italian, he" (Mr. Donaldson) "has read my arguments so hastily as to put them into a different, and what appears to me a contradictory form. But it would require too great a space to enter into the necessary details in explanation on the present occasion." A more modest man would have found less difficulty in accounting for the fact, that my views and his own do not exactly correspond: but Mr. Key's standing rule is—if Mr. Donaldson agrees with me, he must have copied from me; if he differs, he has read my writings too hastily!

There are still some minor misrepresentations in Mr. Key's book, to which it is my duty to reply.

In p. 6 he says: "A portion of a note (p. 275) on a subject scarcely connected with the business of the chapter, has a marked resemblance to two other articles in the *Journal of Education* (iv. 356) and the *Penny Cyclopædia* (v. *Arsis*); and, indeed, the whole of this note, though professing to open a new discovery, teaches what has been taught in the Latin lecture-room of University College ever since the year 1828, and I believe elsewhere also." My answer to this insinuation is simply this: that I had never seen the articles referred to; and if I had seen them, I should still have thought it necessary to write my note; for, on verifying Mr. Key's references, I do not find any anticipation of my doctrine; and Mr. Long,

to whom I communicated the substance of that note, as far as my own doctrine was concerned, professed that it was new to him; and yet he was the editor of the *Penny Cyclopædia* and of the *Journal of Education*.

I find another of Mr. Key's assumptions in pp. 8, 9, where he speaks of some remarks about *annus* and *anna* as an extract from his article on the Roman Calendar in the *Dictionary of Antiquities*. The only ground for this assumption seems to be, that as I wrote the preceding article, on the Greek Calendar, I must have read his paper, which immediately follows. It may be Mr. Key's practice to be always perusing his own writings; but I can assure him that I have never looked at my own article on the Greek Calendar since I corrected the press of it, and that I was unacquainted with my neighbour till he called my attention to it. For the similarity between his remarks and mine, which is not very great, I can easily account; we probably derived our common-places from the same sources. The author of a book called *Varronianus* might be supposed to know that *annus* had been explained by Varro, *L. L.* vi. 8, p. 76. Müller: for the rest, if it is necessary to adduce a secondary authority, I may have been indebted more or less to the *Lexicon Philologicum* of M. Martinus (s. vv. *annus* and *perenna*); a work perhaps not unknown to Mr. Key. But, according to this acute critic,—at once accuser and judge,—I have not merely copied, but my *extract* is not in point; “for though the special character of a year is that it returns like a ring into itself, a river, whatever its motion be, does not find its way back to its own source.” I dare say Mr. Key thought this a great stroke of humour; for, on the strength of it, he calls my explanation of the Tuscan word *ril* “somewhat unsatisfactory work.” I have been studious of brevity throughout my little book, and especially in the lexicographical part of it; and having shewn the connexion of *ri-l* with words implying the motion of running water, I merely added, “how well suited this connexion is for the expression of time, need not be pointed out to the intelligent reader;” and then I went on to establish the connexion between *annus* and *perennis*, a word especially applied to the motion of water. If I have expressed myself too concisely, I am prepared to vindicate my paternal rights, on

the Platonic principle (*Phædr.*, p. 275 E), by expanding, for the benefit of Mr. Key, the outline which I have given in pp. 122, 3, 4.

Without entering at length into the question, how far the ideas of progressive time and recurrence are connected with our conceptions of the regular flowing of water, we may easily satisfy ourselves of the fact, that every language abounds in outward indications of such a feeling. With regard to the connexion of progressive time and recurrence, as the cycles of hours and seasons are always recommencing, it is plain that our first notions of progression in time must be nearly allied to, if not identical with, those of recurrence; and there is no object presented to the senses which is more likely to suggest the idea of the course of time, than the noiseless but unceasing flow of the running stream. Every language abounds in familiar illustrations of this feeling. In Latin, for instance, the word *labi* is applied indifferently to the motion of water and the lapse of time; the same is the case with *fluere*; and every one knows the double signification of the word “tide.” But an etymological analysis of the words which imply “flowing,” “progression,” and “recurrence,” will demonstrate, as far as such matters admit of proof, that these ideas are connected by the link of association.

The particle which, in the Latin language, expresses return, reversion, or recurrence, is that which forms the first syllable of these very words—namely, the prefix *re*. I have elsewhere shewn, that in the Indo-Germanic languages the root *ra* or *re* denotes properly motion in a direct line. Now, the idea of motion in a direct line is the idea of perpetual recurrence, for the line is a series of points with evanescent intervals; and this again is the idea of progressive time. Hence, as speech is co-ordinate with reason, the root which expresses progression also expresses recurrence.

In the Greek language, the particle which expresses recurrence or reversion is *va-* or *a-va-*. Whether this is the same as *ra*, I will not pause to inquire. But it will not be denied that while *νέομαι*, *νίσσομαι*, *νόστος*, &c. express return and recurrence, which is also conveyed by the prefixes *a-va-* and *va-*, and while *νέος* implies *change*, which is included in the



idea of motion (*N. Crat.* p. 62), the words *ρέω*, *vâμα*, &c. express the onward progress of the stream.

It is obvious, then, that the ideas of recurrence and progression meet in the roots *re* and *na*. Now, our conception of “a year” is made up of these two ideas. It is, therefore, most reasonable to expect, that in the Indo-Germanic family of languages one or both of these roots would be found in words signifying that period, which includes all the changes of the seasons—which is always progressive, yet always recommencing; always changing, but always resuming its identity. There is scarcely any reason for doubting, that in the old Etruscan language *ril* signifies “a year.” Here we have the root *ri* = *re*, with the affix *-l* = *d* (comp. *ῥί-διον* with *fi-lius*, *Virgi-lius* with *Virgi-nius*, and the patronymics *Servi-lius*, &c. with *Ἀτρει-δης*, &c.); so that the word signifying “a year” corresponds in formation with *ῥεῖ-τον*, *ῥεῖ-θρον*, &c., denoting the motion of water. And with regard to the Latin synonym *annus*, more anciently written *á-nus*, it is equally clear that this word denotes at once the “ever-flowing” (*ἀέ-ναος*), and “the ever-returning” (*ἀεὶ νεόμενος*). Accordingly, *á-nus* stands on precisely the same footing as *á-ril*, the god, as it seems, of the Tuscan year.

Similar remarks might be made about *per-ennis* and *ju-gis* = *diu-gis*; but this point has been already examined by Döderlein in the first section of his *Lateinische Synonyme*.

When, therefore, we read that *ὁ κύκλος τῶν ὥρέων ἐς τὸντὸ περιῶν παραγίνεται* (*Herod.* ii. 4), such expressions, referring as they do to the recurrence and recommencement of the year, do not at all interfere with the notion of progressive and continuous motion and advance which we attach to the definition of time. On the contrary, they are intimately connected, if not identical; else how could the profoundest of poets express himself thus?

κάτισθι μὴ πολλοὺς ἔτι  
τροχοὺς ἀμιλλητῆρας ἡλίου τελῶν.\*

So that Mr. Key's puerility about the opposition between a ring and a river merely exhibits his own want of depth and accuracy.

\* *Soph. Antig.* 1065.



Again, in p. 44, I find the following note: "Mr. Donaldson classes  $\kappa$  with  $z$  and  $\gamma$ , as subsequently employed by the Romans. This is a strange error from one who has written some fifty pages on the old Roman language. The old inscriptions of the language offer abundant examples of  $\kappa$ , which, in fact, began to disappear just as  $z$  and  $\gamma$  came into use." Any one, merely reading this note, would suppose that I had been speaking of the age of letters as determined by inscriptions. I said nothing about the inscriptions; though the oldest inscriptions in which  $\kappa$  occurs are subsequent to the connexion between the Romans and the Greeks; and some of the grammarians even mention the *ludi magister* who added  $\kappa$  to the Latin alphabet (see Schneider, *Elementarlehre*, p. 289, *sq.*). I was speaking of the original Latin alphabet, from which, according to my theory, the letter  $\kappa$  must be excluded.

In p. 78 Mr. Key has a note, to which I have already referred; he says: "A recent writer imagines that *postea* and *quæ* owed the length of their final vowels to previously existing forms, *postea*c, *quæ*c. To this view there is the fatal objection that *is* and *qui* are never demonstratives, and this affix is only added to demonstratives." Any one who had not read *Varronianus* (pp. 91, 233) would suppose that the author, who is here the object of Mr. Key's sneer, was unacquainted with Mr. Key's views on the subject, and had thrown out an unsupported conjecture. Those who will make the necessary reference to my book, will find that I knew and formally rejected Mr. Key's explanation as "having nothing to support it;" and that my own view rests on the following induction:—*Post-hæc* = *post-hæ-c* occurs in Latin; also, *quā-propter* = *quæ-propter*, and *mea refert* = *meæ rei fert*, according to Verrius Flaccus (*apud Fest.* p. 282, quoted by me p. 233). Therefore  $\bar{a}$  may represent  $\æ$ . Now,  $\æ = ai$ , and as in all other cases in Greek and Latin the neuter plural of the nominative and accusative ends in  $\check{a}$ , the only way of accounting for  $\æ$  in *quæ* = *quai* is to suppose that the additional vowel stands as the representative of a lost guttural fulcrum; and if this explains *quā* = *quæ*, it may also explain *eū* in *postea*, &c.; for there is no reason why we should not have *post-ea-c* in old Latin as well as *post-eśa-k* in Oscan. Mr. Key's objection is *fatal* only to his own pretensions as

a philologer. Not to speak of the actual occurrence of the forms *eis-ce* (Plaut. *Mercat. prol.* 91), *ejus-ce* (Aul. Gell. lemm. c. xiv. l. iii.), &c., all pronouns were originally demonstratives; and the pronouns *qui*, *si-c*, *hi-c*, *i-s*, are four forms of the same pronominal root, signifying relative proximity, in which the guttural element has successively degenerated, according to a law well known to philologers. Now, the affix *ce* = *ci* (*ci-tra*, &c.) is only a fifth form of the same pronominal element; and I am unable to see why it may not have appended itself in the infancy of the language to *qui*- and *i-s*, as well as to *hi-c* and *si-c* (see *N. Crat.* p. 170, &c.). Mr. Key has not ventured to deny that *post-illā* may have been *post-illa-c*; and in endeavouring to explain these forms by a comparison of *post-quam*, *ante-quam*, he has not observed that *quam* is here not an accusative after *post*, *ante*, but the conjunction *quam* = "than" (*i. e.* the locative case of the relative), just as in *posteaquam*, *anteaquam*.

In pp. 5, 6, Mr. Key endeavours to annihilate my 8th and 9th chapters by citing a *single* instance from each, which he represents as samples and specimens of the whole. They shall serve as samples and specimens of Mr. Key's abilities and philological attainments.

After an imperfect reference to what I have said about the confusion of ablative, genitive, and objective notions in the cases ending in *m* and *d* in Latin, and *ν* and *ς* in Greek, he says, with a sneer, "I know not why he omits in this category the nominative and vocative, seeing that his evidence bears still more strongly upon them. Now, such a doctrine once admitted nullifies a large half of the syntax of language." The last remark falls harmlessly on an author who has endeavoured to explain the antagonism of etymology and syntax (*N. Crat.* pp. 52, *sqq.*). With regard to the nominative, I believe most scholars and logicians will agree with me in thinking that it is *subjective* rather than *objective*; and the vocative, if the Professor of Comparative Philology will excuse me for giving him such a piece of information, is no case at all, but either the nominative used interjectionally, or else the mere crude-form of the noun.

"As a specimen of the mode in which our author conducts his analysis of the Latin verb," he selects the explanation of

*ama-ui* as derived from *ama-fui*, which I have taken, with some important confirmations, from Bopp's *Vergleichende Grammatik*, duly referred to in the preceding page (p. 252). Now we have Mr. Key in a dilemma. Either he, a Professor of Comparative Philology, is unacquainted with Bopp's Comparative Grammar (and, to say nothing of my reference, this would not be very creditable to him); or, knowing that the theory was Bopp's, he attributes it to me, because he thinks he can raise a laugh out of it. But is the theory so ludicrous? Mr. Key says that as *fui* was originally *furi*=*fu-fui*, therefore, by my own theory, *fui*=*fu-fui*=*fu-fu-fui*, and so on *ad infinitum*. Reasoning worthy of Mr. Key's clearness of perception. For his argument amounts to this: if *fui* was a shortened form of *fu-vi*=*fu-fui*, therefore it was a shortened form of *fu+fu+fu+fu+&c.+i*. Q. E. D.! On the same principle there must have been an original re-[*n*]-plication (*n* being infinite) of every re-du-plication; so that when Horatius ran away from the Curiatii, he must in those early days have expressed his act with the stammering perfect *cu-cu-cu-cu-cu(+&c.)-curri*. No one but a philologist can fully appreciate the portentous blunder which Mr. Key has committed here. *Fu-vi*=*fu-fui* is the real perfect of *fio*. This reduplicated form has been shortened, like many other Latin perfects, into *fui*, and in this state was employed to make the perfect of the vowel-verbs, and of *possum*, &c. This, I conceive, is certain; and the doctrine gains much confirmation from my view of the forms *arcesso*, &c. Ridicule is a dangerous Warnerian implement in the hands of such men as Mr. Key; and many an engineer of his calibre has been thus hoist by his own petard.

I now take my leave of Mr. Key; and I can assure him that I have not without the most painful reluctance been dragged into this controversy with him. I have not said much about the unfairness and want of generosity which characterise the whole of his procedure, and the tone of ridiculous assumption which he has thought fit to adopt; I have also kept silence on the subject of those peccadilloes which do not immediately concern his attack upon me. But I do not promise the same reserve in future; wherefore *desinat maledicere, malefacta ne noscat sua*. Looking merely to the un-

importance of my little book, and the slight value of my alleged obligations to him, it may be supposed that he has to a certain extent succeeded in exciting a *fluctus in simpulo*; but, in thus reviving the old question as to what is and what is not plagiarism, he has rather approximated to the harmless efforts of the man who tried to create a storm by stirring the German Ocean with a tea-spoon. If we are to encourage the jealous vanity of those who consider their own writings as the only alembicks from which knowledge can be distilled, we must, in the end, sacrifice the interests of literature on the altar of individual selfishness: but the world has not hitherto paid much attention to charges which, though often advanced, have been very rarely sustained.

The work which Mr. Key has selected as the object of his vituperation was originally designed to be an article in a review; and although it has grown into a little book, it still remains, what it was, a general survey of the knowledge now before the world on the subject of Latin philology, regulated by my own theory, and extended by the results of my original investigations. It is not the work on which I rest my claim to be considered an original philologer; on the contrary, it is merely a supplement to the *New Cratylus*. Those, however, who are competent judges will easily convince themselves, that, whatever may be the merits or defects of *Varronianus*, they would have been just what they are, if the Professor had never lived or written. The motives which have impelled Mr. Key to this imprudent aggression are sufficiently transparent; but he will find, sooner or later, that the disparagement of others is not the best road to fame; and one of these days he will regret that his first avowed publication commenced with a wanton and calumnious attack on the only scholar who has mentioned his name with commendation.

THE END.



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